these occasions have been from 300 to 800 times; sometimes one, and sometimes another being used, according to the states of the air or moon, or other circumstances.

The observations here recorded, were made principally from the year 1787 to 1798 inclusive, and they are given in the order in which they occurred with all the phenomena noticed each time, and notes of correction added from subsequent observations respecting stars mistaken for satellites, or satellites wrongly numbered. For Dr. Herschel always added a configuration to his descriptions, in order to avoid mistakes, and in general made, by previous estimate, a sketch of the places where known satellites might be expected; some misconceptions were unavoidable, in consequence of the interruptions to his observations from cloudy or moon-light nights, as well as from small stars that unexpectedly interfered.

From all the observations thus made, and from accurate measures taken by different micrometers, the author infers the nodes of the two first satellites to have nearly the same longitude of $165\frac{1}{2}°$; that their inclination is also the same, $78° 58'$; and the motion of both, from their ascending node to the greatest elongation, retrograde. The periodic time of the first is found to be $8^d\ 16^h\ 56^m\ 5^s$; and that of the second $13^d\ 11^h\ 8^m\ 19^s$; their distances at greatest elongation $36''$ and $48''$ respectively.

To these determinations respecting the orbits of the satellites, Dr. Herschel adds his estimate of the distances to which they must move from the body of the planet before they become visible by his 20-feet telescope. The first requires to be at more than half its greatest elongation. The second also becomes invisible when it is within half its greatest elongation. If there be an interior satellite, as large as the first, he imagines it would be visible through so small a part of its orbit, as not to be seen for two nights together.

Notwithstanding this difficulty, the author thinks he has seen an interior satellite. And with regard to exterior satellites, though nothing has been correctly ascertained, the number and positions of different objects recorded is such, that the author enumerates as far as a supposed sixth satellite.

An Account of some Experiments with a large Voltaic Battery. By J. G. Children, *Esq. F.R.S.* Read June 15, 1815. [*Phil. Trans.* 1815, *p.* 363.]

The battery with which these experiments were made, has 21 cells, each containing about 40 gallons of water, to which was added a mixture of nitric and sulphuric acids, at various intervals, beginning with $\frac{1}{40}$th, and ending with $\frac{1}{20}$th part of the water. Into each cell was immersed a zinc plate, with a pair of copper plates, one on each side, but connected together at the bottom, and also duly connected to the zinc in the cell adjacent. Each plate had 16 square feet of surface.

The first series of experiments were on the comparative liability of

different metals to be ignited by the power of this battery, by joining equal lengths of any two wires in the same line, and making the circuit through both thus connected. In the comparison of platina and iron, which of all metals are most easily ignited, the difference was so little, that their comparative ignition altered during the experiment in consequence of apparent difference in their capacities, as well as conducting powers. Of other metals, gold and copper were nearly equal, and far less easily ignited than the former. After them stood zinc; and last of all silver. Mr. Children observes, that the order of these metals, as conductors of electricity, nearly follows that of their powers to conduct heat.

When this battery was excited to its utmost, it ignited $5\frac{1}{2}$ feet of platina wire one ninth of an inch in diameter.

A bar of platina, one sixth of an inch square and $2\frac{1}{4}$ inches long, was heated red, and fused at each end.

A round rod, one fourth of an inch in diameter and $2\frac{1}{2}$ inches long, was heated bright red throughout.

Oxide of tungsten was partially reduced.

Oxide of tantalum was partly fused, and of a reddish brown colour.

Oxides of uranium and titanium were fused, but not reduced.

Oxide of molybdena was fused and reduced.

Oxide of iridium, with osmium, was fused to a globule.

Pure iridium was fused into an imperfect globule, having specific gravity 18·68.

Of earthy bodies, ruby and sapphire were not fused.

Blue spinel ran to a slag.

Magnesia was agglutinated.

Quartz was not affected.

This opportunity was taken by Mr. Pepys of attempting the conversion of iron into steel by union with diamond, which appeared to have been accomplished. A wire of good soft iron, having been slit longitudinally with a fine saw, the slit was filled with diamond powder, and the whole having then been wrapped round with a piece of muscovy talc, was bound together with a fine iron wire. Although the wire thus prepared was by no means intensely ignited, and for no longer a time than six minutes, no part of the diamond powder was to be found after the experiment, and the iron was found converted into a sort of blistered steel; for it was hardened by quenching while hot, so as to resist the file, and to scratch glass with facility.

Beside the experiments above made with three plates connected in succession as a powerful battery by the action of acids on their surfaces, a trial was made whether at the moment of contact between very large metallic surfaces, any degree of ignition could be rendered visible. All the zinc plates were connected together as one zinc plate, and also all the copper plates connected as one copper plate. A communication was then made between the two sets of plates thus connected, but *not immersed* in a fluid, and all the electricity supposed to move in consequence of the contact, was made to pass through a wire of platina $\frac{1}{3000}$th of an inch in diameter, and about

$\frac{1}{70}$th of an inch in length; but there was no appearance of ignition, although the same wire may be instantly ignited by a single pair of one inch plates immersed in a weak acid.

On the dispersive Power of the Atmosphere, and its Effect on astronomical Observations. By Stephen Lee, *Clerk and Librarian to the Royal Society. Communicated by* W. H. Wollaston, *M.D. Sec. R.S.* Read June 15, 1815. [*Phil. Trans.* 1815, *p.* 375.]

Although the appearances of colour given to low stars by atmospheric refraction be very well known, the comparative degree of refrangibility of the differently coloured rays does not appear to have attracted attention in proportion to its important effects on delicate astronomical observations.

The author endeavours to point out some of the principal errors that may arise from making allowance for mean refraction without due discrimination of the kinds of colour observed. It is evident that stars of different colours will require different corrections in observations of their altitudes.

The apparent altitude of the sun will also vary, according to the coloured glass employed in viewing its disc. For since there must, in fact, be several images of the limb observed at small distances from each other, it becomes a matter of choice which of them shall be selected by the kind of glass used; and it is possible, that to their cause may be ascribed the discordance which exists between the observations of the solstices, and possibly some disagreement between different observers.

Mr. Lee also suggests, from this source, an explanation of the apparent projection of Aldebaran and other red stars upon the surface of the moon,—a phenomenon that has been frequently noticed, but not yet understood. For if Aldebaran be nearly in contact with the upper limb, since the white light of the moon will be elevated more by refraction, it is evident that the stars may thus be made to appear within her disc a few seconds before or after contact.

The author refers to a great number of observations that he has made on Mars, Venus, and fixed stars; from which he infers the quantity of dispersion of light to be between one sixtieth and one seventieth of the total atmospheric refraction.

He also adds several remarks on certain alterations in the mode of making astronomical observations, by which the results deduced may have been affected, especially with reference to those of Dr. Bradley; noticing particularly the period when Hadley's sextant came into general use, and with it the employment of glasses variously coloured, which were soon applied to other instruments.

Mr. Lee concludes with suggesting such precautions as may lead to a more correct knowledge of atmospheric refraction, hoping that the subject may be pursued by astronomers more favourably situated than himself for such an investigation.